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ABSTRACT

A major debate exists in the neuropsychology community concerning whether case study is preferable to group study of brain-damaged patients. So far, the discussion has been limited to the advantages and disadvantages of both methods, with the assumption that neurolinguists pursue a single goal attainable by one or the other method. Practical considerations determine the substance and methodologies of research, and they often influence and constrain the questions asked, contrary to a popular myth that the research question comes first. A historical review of case and group studies and their contributions to the development of neurolinguistics, and of some work in progress or recently completed, including some studies using a hybrid design, reveals a wide range of options between the pure case study and ideal large group study, with specific benefits found in each. There is a logical progression to the kinds of studies the behavioral scientist can do: first, a phenomenological study, either case or group, to describe what may be pertinent to the topic; then, examination of the phenomenon's frequency of occurrence, its elements, and perhaps their interactions; studies of different subgroups; and finally descriptive or theory-driven group studies of the phenomenon's universality. (MSE)

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THE GREAT NEUROLINGUISTICS METHODOLOGY DEBATE

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Currently a major debate rages in the neuropsychology community as to whether the case study is to be preferred over the group study of brain-damaged patients.

The debate first became public at a symposium at the Academy of Aphasia in 1982. Papers from that symposium have been published in several issues of Brain and Language in the following years (e.g. Poeck, 1983; Schwartz, 1984; Caramazza, 1984). Moreover the new journal Cognitive Neuropsychology is devoted to highlighting and exemplifying the benefits of the case study.

Because of provocative statements that not only recommend the case study, but also criticize the group study, practitioners whose primary work has been on group studies have felt obliged to defend themselves. This has led to a healthy evaluation of methodology in neurolinguistics, but the discussion to date has been at the level of advantages and disadvantages of either case studies or group studies. The assumption in such a discussion is that neurolinguists pursue a single goal and at the end of the debate we will know whether the case study or group study will best get us there. I question this assumption.

For a number of years I have been interested in the issue of how research questions develop, and how the techniques and methodology one uses influences both the questions one asks and the answers one gets. Even by framing my questions the way I do, I am contradicting the standard myth about the way research is conducted.

As we all know, there is the myth about science that a question comes to us through inspiration and/or via the muses, and then we sit down to design the best experiment for answering it. Curiously enough, the muses regularly ask questions which appear to be answerable using the techniques we are already familiar with, the equipment we have in our laboratories, and the brain-damaged populations we have some access to. So there is invariably a range of pragmatic factors that determine what research scientists pursue (and what we choose not to pursue), and the techniques and methodologies we use to answer our questions. Moreover, I have a conviction that the methods we use influence and constrain the questions we ask, that is, that the procedure of the myth is in fact often reversed.

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In order to gain some perspective in the neurolinguistics debate, I determined first to review the history of case and group studies, as they contributed to the development of neurolinguistics, and then to reflect on the work I have conducted in the field, in order to illuminate my thinking by using the data I know best, that is, the research process I have been involved with.

Historically, of course, neuropsychology evolved as a discipline with a large number of case studies advancing the field. However in a sense it was a group study which triggered the development of the field, namely Broca's observation that over a series of patients with language disorders, it was characteristically the left hemisphere which was damaged and not the right. Such an observation could not have been made, it is clear, on the basis of a case study. Also, already at the end of last century there were group studies of the descriptive sort. For example there was the work of Séglas (1892) on the language of mentally disturbed patients, which included descriptions of the language of dementia. These sorts of group studies were descriptive of the range of phenomena observed, but they were not in the least quantitative like the group studies which were to follow in the 20th century.

With the increasing dominance of behavioral psychology, especially in the United States, group studies became the preferred mode of aphasiological work. Numerical scores could be given for each patient's performance on a task or a set of tasks, and then statistics could be applied in order to test whether there were differences between two groups or two conditions. Naturally these groups were never too large when they involved brain damaged subjects, because no given institution sees great numbers of such patients.

As the field of aphasiology developed aphasia subtypes were identified. Thus it soon became appropriate to study presumably homogeneous groups of patients within a subtype, or compare one subtype with another, rather than simply testing "aphasics". And finding a large group of a given subtype was often pragmatically difficult, so groups sized 6-15 were considered acceptable.

Along with these group studies, fascinating individual cases were often still reported, usually by behavioral neurologists and their colleagues, across the century. By definition what makes a case unusual or interesting is a pattern of dissociation or association which has not been seen before and is not trivially obvious or explainable. Only recently, in the time of the Great Debate has there been a turn to the case study of the individual case that is not necessarily unusually "interesting". Practitioners such as Caramazza who choose the case study method maintain that

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virtually any patient with a given neurolinguistic breakdown will prove of interest in extensive testing of hypotheses. Moreover they maintain that the group studies standardly done in aphasia bring together patients who may have a similar diagnostic syndrome, but in fact differ in so many ways that interpretation of combined results from a number of patients hides interesting phenomena and suggests as truths findings which are due to something other than the syndrome.

This question of whether the case study or the group study is to be preferred clearly interacts with my question of how different techniques and methodology interplay with the sorts of questions we ask and the answers we get, a point I will return to below. First however I want to talk about a related set of questions, that of the issue of theory-driven versus descriptive or, as it is sometimes called, pre-theoretical research. Theoretical work generates and elaborates models; descriptive work provides the data from which theories can be constructed, and tested. A major impetus to this question of whether research should be theory driven or whether it should be descriptive, results from the interdisciplinary nature of neurolinguistics.

Indeed, one of the reasons why neurolinguistics is as vital a field as it is, I suspect, is because it results from the convergence of three quite different fields of knowledge: linguistics, behavioral neurology, and psychology. Because the training for each of these fields is substantially different, and the ways in which one acquires knowledge in each of the fields is different, the sorts of questions and the methodologies used by each type of neurolinguist vary substantially.

The behavioral neurologist is trained to see each patient as an individual and to search for the cause of pathology via a listing of the spared and impaired abilities, and to search for the parameters of a patient's history which have proven to be related to the symptoms in question. Clearly this sort of training results in the predilection to appreciate what is special about a given patient, and to do case studies of the sort which demonstrate unusual dissociations or associations of disability.

At the same time, very quickly in the training of the good clinician (I include in this term speech pathologists and neurolinguists doing clinical work and psychologists and neuropsychologists, as well as more direct health care providers such as physicians) something we call "clinical intuition" develops, whereby the clinician can appreciate through some gestalting ability that the patient in front of her or him is similar to another patient seen previously. This appreciation permits the physician to close in on a specific set of questions in working out the logical connections which may rule out or rule in a given diagnosis. Thus the clinician also

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has a sense of the individual patient as the member of a group bearing a diagnostic label.

The experimental psychologist, by contrast, is trained to disregard any sorts of "clinical intuition" since they probably result from prejudices and biases. Rather the focus must be on developing hypotheses for truths which will hold across individuals, and thinking through ways to test them. The psychologist does not expect all subjects or patients of a given subtype to perform in a certain way, since human beings are so complex. Rather he or she relies on statistics to determine when trends are significant, i.e., when findings are very unlikely to be due to mere chance. Thus the psychologist is prone to doing large group studies (indeed the groups should be as large as possible) and has, as a rule, great disdain for the case study since any case may be an exception to the general rule.

Note that there is some circularity in the argument that large group studies are necessary in psychology because you can only do statistics on data from large groups. In fact statistics as we know them were developed explicitly in order to conduct large group studies; only today are a new set of statistics being developed for single case studies. Rather than randomizing the selection of the population (which is of course another myth; it never really happens in even the largest group studies), one randomizes the multiple results for analysis.

The linguist, by contrast to both the psychologist and the behavioral neurologist, is trained to observe and infer patterns in corpora of data. Sometimes these patterns are relatively explicit or surface patterns; other times they are "deep" and must be uncovered. For the linguist, truth lies in finding the elegant or parsimonious description of such patterns which accounts for as much of the data as possible, either data at hand or data which may in future be presented. With Chomsky's revolutionary insights of the late 50s ja 60s, linguists have come to believe in a myth of our science, that in linguistics one should be able to choose between two alternative theoretical explanations for a given phenomenon. In fact, however such a procedure is virtually never practiced because linguists seem to prefer to move on to new topics rather than worry old ones. The linguist's approach does not particularly predispose to using either case or group studies. Patterns can be deduced in data from either type of study, though I suspect that the types of patterns which will be evident may differ somewhat, since the case study will demand or suggest more micro-level analyses, while the group study will present more macro-level possibilities.

Within the myth of how psychology is practiced, one works down from a theory. Thus theory-driven work is considered more valuable than "merely" descriptive

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work. Linguists too are concerned to present refutable statements, but the form which the refutations may take are not as rigorously delimited as those of psychologists. In psychology, the standard form for research is setting up two alternatives, and then determining a way to choose between them. For the behavioral neurologist whom I have stereotyped as prepared to spot the educational and interesting case, or for the linguist prepared to infer patterns of language behavior in individuals or across groups of individuals and propose theories based on them, the concern for choosing between alternative hypotheses is simply of less interest. Rather the concern is for providing careful descriptions of new phenomena which the theories must eventually explain.

My own work has been primarily with group studies, of larger and smaller groups, but it has also included some case studies, and several of my newer interests, namely the Cross-Language Study of Agrammatism and the Neuropsychology of Talent and Special Abilities, virtually demand the use of the case study. In addition to the cross language study of agrammatism and the Neuropsychology of Talent and Exceptional Abilities, my interests have been in two areas: the neurolinguistics of bilingualism, and the language changes of healthy aging and dementia. Within two of these four fields, bilingualism, and language in aging and dementia, I have been involved in quite a number of studies; the Cross-Language Agrammatism Study by contrast is a single major project involving many colleagues on syntactic production of agrammatic aphasics in 14 languages, and the work on the neuropsychology of talent is in its early stages. There our immediate goal is a co-edited book on the topic to bring together what work there is on talents such as calculating, hyperlexia, chess players, music, art, and learning a foreign language like a native after puberty. The long term goal is a series of case studies to permit us answers to the questions about how the brain is organized for special talents.

Our work in bilingualism and on aging by contrast, has been virtually composed of relatively large scale studies of one or another sort. The groups involved were larger in the cases of healthy bilinguals and healthy elderly, and smaller in the cases of harder to get populations such as the demented patients and the aphasics we contrasted with them. The Cross-Language Agrammatism project is a curious combination of the case study concept and the group study concept as I will elaborate below.

My methodology for thinking through this paper was as follows: I simply reviewed the research I had done, in each instance asking what the research question

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had been, what type of study it had been - initially I simply asked whether it was group or case study -and what the results or answer had been. Of course at the time that we designed the studies, it often seemed that our methodology was the only appropriate one to use. In thinking for this paper, however, I pushed myself to ask if we had changed certain parameters of the methodology would we have had to reshape the questions and would we have obtained different results?

As to the type of study, although I initially simply intended to determine whether it was a group or case study, I soon found myself differentiating between large groups and smaller groups, and between different types of case study. I also considered the order in which various components of the scientific method had in fact been carried out, e.g., whether the data were collected after the question had been posed, or whether the question was posed on the basis of data already collected by someone else. I suspected it had also made a difference whether the tests were designed to answer the specific question, or whether previously used tests of other investigators or of our own were employed to give the answer. One question I began to ask was whether the source of the research question was from clinical experience or from experimental experience, whether one of our studies followed up another one in linear fashion or whether it developed to fill a gap in order to be comprehensive.

I won't be able to review all my research in this paper, but I have selected examples of sufficiently different techniques and methodologies to justify my points.

The largest group studies I've been involved with have been the studies of healthy language and aging. The basic question which motivated Martin Albert, my collaborator, and myself was: Are there changes of language with healthy aging? Predicated on the assumption that there would prove to be subtle changes, we asked: If there are changes, what is their nature? Thus initially our research called for pure description. And we set up a major research protocol to look at all aspects of language: laterality, naming, comprehension, automatic speech, discourse, and metalinguistic abilities. Our first set of papers described the quantitative and qualitative changes we saw in some language abilities - naming, comprehension and discourse in particular - and the lack of changes we saw in others - laterality and automatic speech, for example.

In the next phase of this project, as we have a better sense of what changes there are and which do not occur, we have begun to ask what the changes relate to. For example, do they relate to the individual's history of alcohol consumption, or of

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bilingualism, or do they relate to gender differences across the lifespan? Do they relate to performance on other language tasks, or does aging affect different language tasks independently? And do changes in language performance in the elderly relate to non-language neuropsychological abilities such as memory and attention?

In the third phase of this project we are developing theoretical hypotheses to test, so that we can use our data on naming in aging, for example, to learn more about the naming process and the processes of cognitive change related to age.

For the extensive set of projects we undertook we recognized that we would need large numbers of subjects in order to see the relatively subtle changes we expected. We assumed that because language abilities are so diverse, and aging so individualized not all elderly would show them. Thus we would need to look for means, and work with statistical differences rather than the striking dissociations one may see as the result of frank brain-damage. Our statistician told us we needed to have a sixteen person cell for each gender, in each of the four decades we planned to test, the 30 year olds, the 50 year olds, the 60 year olds, and the 70 year olds. Although many before us had done studies of language in aging by comparing one younger adult group with one older adult group, we assumed that it would be valuable to get four data points rather than two in order to see whether there was a progression of changes with ages or a drop-off, presuming changes were found. Our initial assumption proved true that there would be substantial intra-individual variation in language performance, which would mask patterns of change across the larger group. Indeed, as in many of cognitive studies of aging, our data evidenced an increase in standard deviations with increasing age.

I will not be able to go into detail on our findings here. In brief summary, however, I will report that in certain of our neurolinguistic and linguistic realms we found no differences with age, such as the tests of laterality and automatic speech; on other tasks such as naming (and this was equally true of common nouns, proper nouns, and verbs) performance declined with some qualitative changes in response type. For comprehension, by contrast, while correctness scores declined, there were no qualitative differences seen despite the fact that we were explicitly looking for them in those tests. With discourse production, we saw an increasing elaborateness with age which was linked to better performance. In order to make such summary statements, of course, I ignore the individual differences and the range of variation. So, using such a methodology I am able to answer the question of whether there exist subtle changes for population groups, I am unable to predict what is happening

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for the individual. There is a certain irony in the realization that one cannot generalize from a case study but nor can one generalize to all elderly as individuals from a group study.

Our series of studies on language changes of dementia provide a certain contrast. Originally we had thought we would give the demented patients the same tests we were giving the healthy aging patients. However it soon turned out that the demented patients were performing for the most part so much worse than the healthy aging, that only those tasks which were too easy for the healthy elderly could we use with the demented patients, such as the tests of automatic speech, in which we ask subjects to recite the months of the year or the numbers from 1 to 21. Most of the tests which were interesting enough to provide a range of performance among the healthy subjects would have been unkind to give to the Alzheimer's patients since they either could not have caught on (as in the case of watching a television and pressing a button to demonstrate comprehension) or they would have failed severly (as in the case of the 85 item naming task).

Instead, as I spent more time with demented patients interviewing them, a different set of questions were motivated for clinical purposes.

For example since the literature on language changes of dementia was so sparse when we started, it was important to resolve the apparent contradiction that people reported both logorrhea and muteness in dementia. Over the course of working with patients in different stages of the disease, it became clear to me that logorrhea was associated with a middle stage of the disease, and muteness with an end stage. Indeed I was able to discern a progression of six stages from early to late, which we describe in greater detail in Martin Albert's book The Clinical Neurology of Aging (1984). Interestingly, two of the stages were particularly compelling to the linguist/aphasiologist in me; the early-to-mid stage when the patient looks something like an anomic aphasic, and the mid-to-late stage when the patient looks substantially like a Wernicke's aphasic. This observation led to two additional studies, one to distinguish the empty speech of the Alzheimer's patient from that of the Wernicke's aphasic and the anomic, and the other to test naming in these patients and relate it to that of anomics and normals. Again, this question of how to differentiate the speech production of the Alzheimer's patient from that of the aphasic had a certain diagnostic impetus because I was regularly asked by neurologists to judge whether a given patient was in the relatively early stages of the dementing disease or whether he or she was aphasic from causes other than dementia. I selected a set of language tests to give demented patients over time,

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and I developed a certain clinical intuition which allowed me to evaluate a patient alongside my memory for patients seen previously. The study of empty speech permitted us to quantify impressions (Nicholas et al, 1985). At first I was distressed that out of the 14 items we chose to look at: (including empty phrases, deictic terms, anaphora without antecedents, indefinites, paraphasias, neologisms, and lack of discourse conjunctions) only three differentiated the demented from the Wernicke's aphasics patients. But then I realized that this lined up with everyone's previous clinical sense that the empty speech was really quite similar across the two groups. The differences, by the way, lay in the fact that the Wernicke's patients produced neologisms and verbal paraphasias significantly more frequently than the Alzheimer's patients, whereas the Alzheimer's patients used a broader range of conjunctions, particularly the logical conjunctions such as because and although, as compared to the Wernicke's patients. Had we compared more extended discourse from a single patient with Alzheimer's disease with that from a Wernicke's aphasic, we well might have obtained the same results, although it is also possible that certain of the particular phenomena observed to distinguish the groups might not have obtained with sufficient frequency to permit statistical differentiation.

My clinical work with demented patients suggested a further study. I was impressed with the fact that the comprehension of the mid and mid-to-late stage patients was quite poor, for reasons which appeared to me to be attentional. In the Wernicke's patient, by contrast it is a language area of the brain which is destroyed. It made sense to suggest that reading materials be given the Alzheimer's patient so that there were two modes of input, so memory would be less called upon, and attention would be reinforced. This study was actually conducted as a relatively small group study, with 9 patients each getting the same battery of comprehension materials from the Boston Diagnostic Aphasia Exam in each of three conditions. The choice of a small as compared to a large group here, as in many other instances was pragmatic rather than theoretical; a student was doing it as her honors thesis. However the results were encouraging with seven out of nine patients performing better in one of the written modalities, and the differences reaching significance for the most complex subtests of comprehension. In a case study we might have tested one of the two cases who were not helped by written input, and might thus falsely have concluded that it would not help other patients with Alzheimer's disease.

By contrast to the work on healthy aging, my work in bilingualism regularly involves somewhat smaller groups of 10-15. Interestingly, they would be considered rather large groups if they were studies of brain-damaged subjects. The work on

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laterality in bilingualism presumed that two groups of 24 bilingual and monolingual subjects would be representative of their respective populations. The one bilingualism study which appeared to be based on a larger group, was the review of bilingual aphasia cases in the literature available to us at the time (Albert and Obler, 1978). In that study we looked at 106 cases which had been published, in order to ask the question which had dominated the field for the last century as to which language comes back first in those polyglot aphasics who show differential recovery - that is a difference in aphasia between the two languages which could not be predicted on the basis of the patient's knowledge of the two languages premorbidly. Was it the rule of Ribot that the first learned language returns first, or the rule of Pitres that the language used at the time of the accident returns first, or some other factor such as handedness, or the specific language spoken, or age of acquisition, or age at onset of aphasia? Thus we coded for all the personal history, aphasia type, language history and etiology variables available in order to see whether there was anything else which predicted which language would return first. As it turned out we did get significant results in favor of the rule of Pitres, whereas the rule of Ribot held with merely chance frequency. But we could say nothing statistical about most of the other factors we had been interested in, since the data were too skimpy on any given parameter (Obler and Albert, 1977). With this study, I first learned that what appears to be a large group is not necessarily so if one does not personally control the collection of raw data. Thus we had handedness data for something like only 15 of the subjects, and in many cases the language testing was skimpy, and in only a few of the patient's languages. In a few cases even the gender of the subject was not given. As a result we were unable to find any of the other possible correlations which might have proved of interest.

Indeed the case studies we have done on polyglot aphasics have proved at least as informative. Of course from the case studies one cannot answer questions about how the group as a whole responds, but one can instead find a dissociation, such as that reported in Albert and Obler, 1978, with different aphasia types in each of two languages. Because this is such a rare phenomenon, we were fortunate to locate and test this case. Note that this sort of dissociation can hardly be looked for in a large group study, and yet it is very important for documenting the range of ways in which language can be organized in the brain of the bilingual.

Certainly the largest group study I was involved with was a study of handedness in Israel (Silverbert et al, 1979). As I was coming into the field of neurolinguistics, like many I became particularly aware of how many left handers there were in the

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world. Probably due to the circumstance of my being in Israel I soon hypothesized that Israelis as a group had a greater proportion of left handers than I recalled from my limited upbringing as a right hander in the United States. Such an epidemiological study has to be done on a very large scale in order to have data to compare with other studies in the field, so we tested 2,000 subjects, bringing questionnaires from classroom to classroom. In the end we discovered that there are virtually no handedness differences between Israeli Jews and the rest of the world. The lesson this taught me is that group studies do not necessarily provide momentous discoveries proportional to the largeness of the group!

Because of an age-related finding in the study of Pitres, namely that while the group as a whole followed the rule of Pitres the subjects over age 65 did not, Marty Albert and I decided to consider the relation between aging and aphasia type which had not been studied previously (Obler, Albert, Goodglass and Benson, 1978). This research was conducted on a fairly sizable population drawn from about 600 patients seen by Frank Benson at the Aphasia Research Center. We narrowed our sample to a fairly homogeneous set, selecting all 167 patients who had aphasia resulting from stroke in right handed white males who fell into clear cut diagnostic groups of aphasia, and discovered that the Broca's aphasics were significantly younger than the average and the Wernicke's aphasics significantly older. Although this finding has been replicated in at least 11 aphasia research centers aroung the world, and demonstrated by Miceli et al. to be true for patients with tumors as well, the explanations for the findings are elusive. The interesting explanation of course would be that the brain substrate for language reorganizes itself between the early 50's and the mid 60's. Less interesting explanations would be that humans tend to get different sorts of strokes and tumors at different ages.

In a follow-up study with J.P. Mohr and Lou Caplan (1981) we employed the Harvard Stroke Registry, another seemingly large scale data set, in order to look for correlations between etiology, lesion location, and age. As in the previous study, the fluent aphasics were significantly older than the non-fluent aphasics. The strongest finding for the interesting explanation, that the substrate for language reorganizes across the lifespan, would have been if 60 year old patients with anterior lesions had posterior aphasias. But we did not see such a correlation. On the other hand none of the other correlations held true either. For example, there was no significant finding of increased numbers of posterior strokes with increasing age. We are forced to conclude then that the initial observation that older aphasics are more likely to evidence Wernicke's aphasia still holds true, but our attempts at large group explanation have not succeeded.

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An alternative approach I fantasize, assuming the appropriate volunteer or volunteers could be found, would be to do a case study or a small group study using a pet scan or the cortical stimulation technique longitudinally. Once each year between the ages of 50 and 70 the patient would be tested in order to determine the extent of his or her language area of the left hemisphere, and within the language area, the extent to which anterior and posterior areas were responsible for comprehension and syntactic production. If the interesting explanation is correct and true for all people, it should be possible to demonstrate that it is true for one individual on the basis of a case study. Given our current set of beliefs that the brain substrate for language is fairly fixed after puberty, even a single case would be so stunning as to force us to rethink our set of beliefs about the development of brain substrate for language. But again, to the extent that we wanted to generalize beyond what might possibly be an individual freak case, we would need to do larger scale studies.

The clearest instance in which I needed to do a case study was in learning about the neuropsychology of talent and special abilities. My interest in this topic was piqued when I heard Dorothy Aram talk on hyperlexia, and realized that to study it or indeed to study any talent would be to reverse the standard neuropsychological paradigm of looking at deficits against a background of normal abilities. In studying the neuropsychology of talent one focusses on the talented ability and tries to determine its components, as they relate to the background of merely normal abilities, or in the case of idiots savants, as they relate to poorer than normal abilities. We have attempted two sets of case studies in Boston, one on exceptional second language learners and the other on natural speed readers. The study of natural speed readers has been delayed because they are extremely hard to find if indeed they exist. We do believe we have located at least two. But for one, her overall intelligence is so high we would not be able to find any of the dissociations we would prefer to find, and the other lives at a great distance - another pragmatic constraint. Two others who volunteered demonstrate very fast reading but with poor enough comprehension that they are not what we meant when we said we wanted to study natural speed readers!

With the exceptional second language learner, by contrast, we have been able to locate several in the Boston area and have completed testing on one and almost completed it on the other two. While it might be of interest to study large groups of such subjects, they are simply not available, and in any event intensive work is

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required to look for dissociations between different neuropsychological abilities. In the study of the first subject reported by Novoa et al., we found an interesting dissociation between CJ's overall memory abilities and his memory for language. When given a word list of 20 items, for example, he remembered no more than the normal person upon immediate testing. However when tested two weeks later, he remembered a majority of the words, when the normal can remember only a few. We also found a dissociation between his overall IQ, which was merely normal, and his language learning abilities. The third dissociation we found was among the subtests of the Modern Language Aptitude Battery (Carroll and Sapon, 1959). This test of John Carroll's was devised to test the various skills which go into good language learning ability. Our subject CJ did well on all of them except the ability to abstract grammatical rules. Somehow his language learning abilities permit him to learn language without this ability to abstract rules. Of course we cannot conclude that the next subject we test will not be good in this ability; indeed the fact that this subtest remains in the test battery after Carroll took out all the redundant tests argues that there are subjects for whom that skill is valuable in good second language learning. What we can say is that the ability to abstract grammatical relations is not crucially necessary for skillful post-pubertal second language learning ability.

In addition in that study we found an interesting qualitative result whereby CJ appears to prefer form over function. On the similarity subtest of the Weschler Adult Intelligence Scale, for example, when asked what work and play have in common, CJ reported that they both have four letters, rather than that they are both human activities. When asked what a statue and a poem have in common, he cleverly replied that they both have lines. This focus on linguistic form does not get you many points on an IQ test, of course, but we suspect that it is one of the components which contributes to CJ's superb second language learning skill.

Had we studied a number of exceptional language learners in a group study, many of the specific dissociations we found in CJ might have washed out. After all, we do not claim that all exceptional second language learners have exactly these abilities, but rather that this constellation of abilities makes it possible for CJ, an otherwise normal human being, to be an exceptional second language learner.

Let me turn finally to two examples of hybrids between case and group studies. The first is a study of pragmatic abilities in dementia which developed out of my changing focus of interest within dementia. It is a good example of the sort of small case-group study that some advocates for the case study approach consider ideal. In

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case study, each subject or patient from a small group of very homogenous patients is studied extensively. This permits one to see patterns across the patients because of their homogeneity, but also to exclude as non-universals those points on which patients differ. This study evolved as we realized that no one had considered the pragmatic abilities of end stage dementia, which were reported by Irigaray (1973) and others to remain until quite late in the disease. Around the time I realized it was worth wondering about this, a student came along who was looking for a master's thesis project, and she found this study interesting. The methodology we employed was to do intensive analysis of the pragmatic communication behaviors of as many subjects as possible, in order to document the range of remaining abilities, and to determine when they are used appropriately and when inappropriately. Such a study which sets out to describe the range of behaviors is clearly an early study in a field which has been untouched. Irigaray's 1973 study of language in dementia took a similar tack. No numbers are generated at this early stage, but rather the boundaries of phenomena to be studied are staked out. This could actually have been done on a single case study individually, but one would have no idea how representative it was until a group of cases with presumambly the same diagnosis were studied. And our clinical intuition told us that there would be differences in the sorts of communication behaviors which patients retain into the late stages of Alzheimer's Dementia.

In fact, the way we are carrying out this study is a combination of the case and group study in that each case will first be studied and analyzed individually. One of the difficulties with Irigaray's ground breaking larger scale study (she had 53 subjects total) was that it treated each language ability in a separate chapter. However one could have no idea how two or more language abilities clustered for a given patient. Our strategy in this project of looking at the pragmatic abilities through extensive work on each of 9 patients in order to talk about clustering of abilities within each patient. Then we will compare across patients to see whether there is a given hierarchy of preservation, a given cluster of pragmatic abilities which is regularly spared, or whether there appears instead to be substantial individual differences and dissociations.

The second hybrid between a case study and a group study is less orthodox in methodology. It is the Cross-Language Agraminatism Study which Lise Menn and I are coordinating and which involves my host, Jussi Niemi, and several others in attendance at this meeting. The question we ask in this work is whether the language structures of different languages interact differently with agrammatism.

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Agrammatism, as you know, is that particular form of aphasia in which the patient omits word endings and some functor words; the resulting speech sounds more or less telegrammatic depending on the degree of severity of the agrammatism. Actually theoretically it need not be word endings which are dropped, but the century of study on agrammatism has been largely done in Indo-European languages where word endings mark the inflections; in a language such as Swahili with productive syntactic use of prepositions, as Traill (1972) reports, these also can be impaired in agrammatism.

In order to answer our questions, one could, I suppose, do a study of a bilingual or, better, a multilingual aphasic who had agrammatism in all languages. Ideally the patient would have been fully fluent in all the interesting languages of the world; Chinese, Japanese, Finnish or Turkish; an American Indian language, and several African and Melanesian languages. Waiting for such a patient to turn up proves prohibitive. And to make it worse, only 2 % of all aphasics, I have read, are agrammatics. So, we determined instead to work with colleagues who could locate agrammatics in different languages, and to ask each to do two case studies, to compare them to each other so we could see what individual differences could be ruled out as not due to differences in the language structure, and then compare the agrammatic cases to normal controls in order to see what language errors were mere normal slips of the tongue. The result will be our large sourcebook, currently titled Agrammatic Aphasia: Cross-Language Narrative Sourcebook which will permit us all to do group studies by looking at differences across patients.

Here we have a substantial number of questions which can be answered, looking both at universals and at differences especially between languages. For example we have asked our participants to write about the special features which their agrammatics show in their language; if both agrammatics show a given pattern, we are more likely to assume that it is the language structure in conjunction with agrammatism which brings it about; if only one patient shows the pattern, we ask whether that patient is the more impaired, or whether there are different subtypes of agrammatism in any language, a conclusion we are coming to at this point. We also ask which features are regularly preserved that one would have expected be omitted in agrammatics, given the standard definition involving loss of inflectional endings and functors. Indeed we ask our participants to compose a hierarchy of deletability of morphemes when this is possible.

When there are differences between the two agrammatics, of course, we cannot assume they relate to the language or the degree of the disability; we first ask our

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participants to rule out dialect and education differences and potentially handedness and gender differences. One technique we borrowed from linguistics is to ask what sentence types occur. A finding we have across many languages particularly evident in Finnish, for example, is that the agrammatic patients actually only attempt a limited set of syntactic types. This finding would not have been noticed if one simply performed error analysis, or if one only studied agrammatic patients without having normal controls against which to measure their performance.

Another question we will be able to address by this hybrid case and group study is the dissociability between different language modalities such as reading and speaking and between comprehension and production. Thus we already know that for some of our patients there is a good correlation between agrammatism in reading aloud and speaking, and for others there is no correlation whatsoever. Likewise with respect to comprehension, as has been posited on the basis of certain cases and indeed small group studies, there are patients in our study for whom comprehension of syntax is impaired in ways similar to their production. However there are also patients for whom comprehension is simply not impaired, so we must conclude that comprehension disturbance is not universal in agrammatism, and thus that the syntactic processor for comprehension must be dissociable from the syntactic processor for production at least for some humans.

Finally we ask our participants to address the several explanations of agrammatism which have been proposed over the years in order to see whether we will see different ones supported on the basis of different languages. As you may know there had been functional explanations of agrammatism and phonological explanations as well as morphological explanations and more processing explanations such as ones based on words' position in the sentence and sentence length. Clearly such a question is answerable as we have set up the study, but I do not have the answer to give you today.

Obviously had we done only a case study in each language, we would already be restricted in our conclusions beyond what we can say by testing a pair of patients and a pair of controls. On the other hand had we done a large group study, certain of the dissociations we see in an individual patient and between patients might have been masked due to averaging. So in the end I conclude that this combination of extensive testing along the lines of the case study and attention to the clustering of behaviors in the individual has worked well in this study which also requires relatively small group analysis.

One conclusion I was surprised to come to from my review of the research I had been involved in was that there was a wide range between the pure case study and

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the ideal large group study. For different research projects "large" in terms of group meant a substantially larger number than for other sorts of subjects. Clearly there are pragmatic reasons involved such as the amount of time the investigator will have, and the likelihood of finding a number of a given sort of patient. However I now believe, contrary to the cynicism with which I started working on this paper, that it is not pragmatic variables alone which determine the size of a sample. Rather on top of the pragmatic variables which say that it is harder to find brain-damaged patients than healthy patients, and harder to find specific interesting subcategories of brain-damaged patients than it is to find just any brain damaged patient, there are also compelling scientific reasons to take unusual cases more seriously, to give them greater weight, than cases we believe to be more run-of-the-mill or normal.

Moreover, I realized, the magnitude of the data to be analyzed is often virtually the same in a large study, a small group study, a case study, or some combination of them. When one has a single case, one generates substantially more comprehensive data than when one is simply asking 10 handedness questions from a sample of two thousand. This phenomenon is being formalized and addressed with the development of single case study design and statistics today. The debate which polarizes discussion around case versus group studies, then, is inappropriate on two grounds, first, because there is a viable continuum between the two poles, with variation between the two ends of the continuum, and second because the amount of data to be analyzed can be surprisingly similar.

What the case study buys one, in essence, is the possibility of demonstrating a clear-cut dissociation, and the possibility of claiming that we see one example of how a human's brain operates, and how language information is processed in at least one person. Also, a case study can contradict any generalized assumption that "All human beings X". What the small group or the large group study gives us is population trends, the possibility of predicting the likelihood that the next patient or subject who appears to fall into a group will share those additional characteristics that you have discovered via the case study. It is possible to reveal small but systematic changes in a population that might not have proven significant in any individual case study. Moreover it is important to note that only in doing group studies can one find what we call individual differences, i.e., differences between subgroups of a population; with the single case study, one cannot make claims for group differences. Of course one can do group studies and not look for individual differences or sub-group differences; but if one plans to look, they can only be found

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in the group studies. These constraints on the sorts of findings one can have are essentially the constraints on the sorts of questions one asks. So I conclude that the myth is certainly not fully true, that we ask the questions and then go out and choose the appropriate methodology. Nor is the converse the truth, that the methodology we use fully determines the questions we may ask. Rather there is an interplay between the techniques and methodologies we use and the questions which they predispose us to be interested in, and therefore the answers we will get. At the same time there is an interplay between the questions which come to us based on our previous research or our readings in the field, or insights from the muses even, and the ways they direct us to modify a given methodology so it permits us to approach an answer to the question we have.

There is a certain logical progression to the sorts of studies the behavioral scientist may do. First, in the early stages of approaching a new topic, we need a phenomenological study to describe what may be considered pertinent to the topic or questions. This can just as easily be a case study or a group study. Then we need a study to look at the frequency of occurrence of the phenomenon and its elements and perhaps how they interact. Study of different subgroups can follow. Clearly this requires more than a single case design. On the basis of results from such studies one then designs group studies to see how universal a phenomenon or a cluster of phenomena is. These can be descriptive studies, or they can be theory-driven on the basis of hypotheses.

Writing this paper has made me feel better about the term "pre-theoretical", which in some neurolinguistic circles is said in a derisive tone about work which is not strictly theory-driven. For me, it is the pre-theoretical ends of the field which are the interesting ones to work in, and I now realize that this can be done through case studies in the early pre-theory, and through group studies in later pre-theory. By the time the work gets to be "theory-driven", it has always seemed to me to be too constrained, or "theory-limited" to be of interest any longer. But I hasten to say that that is a reflection of my personal intellectual style and taste; I would hardly prevent a student from pursuing a theory-driven question, or even think the worse of a fellow researcher who preferred to work in that mode.

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